

CONDUCT AND CORRECTNESS IN MATHEMATICAL PUBLISHING

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ABSTRACT. This is an essay in which I try to express my fear about the establishment of a culture of publishing where no one is willing to take responsibility for the correctness of mathematics, and readers finding mistakes in published proofs are stamped as outcasts, because they are deemed to target the reputation of authors and journals.

keywords: Publication of research, correctness, peer review, conflict of interest

1. INTRODUCTION: JOURNAL PUBLISHING FOR MATHEMATICAL RESEARCH

Research belongs, to a larger or lesser extent, to the life of every academic mathematician. Unlike other sciences, the validity of mathematical research depends on calculations and logical steps which derive a new result (theorem) from previously known ones. Nowadays mathematics has grown more complex than the expertise of individual mathematicians, and the knowledge needed to understand and apply a theorem often differs considerably from the one needed to understand its proof. However, if a flaw is found in the proof of a theorem (unless other correct proofs exist), everything building on that ‘theorem’ is potentially also flawed. On the other hand, electronic media have facilitated the communication of research, and therewith also of poor one. This dramatically increases the importance of journals as places to look for reliable material.

The process of publishing in journals briefly goes thus. An author writes up the results of his research in a paper, and chooses a journal to submit. The editor of that journal examines, with the help of one or more referees, the suitability (including correctness) of the content, and decides whether to publish it or not. In latter case, the author may submit the paper to another journal, where the procedure repeats. (See [Bernstein, Bernstein2] for a general code and [Bornmann 2011] for an interdisciplinary survey of peer review.)

As the process has been arranged in mathematics, the referee often knows who the author is, but not vice-versa. This practice is justified by assuming that a referee (who is thought to be a mature, mathematically and ethically responsible individual) would not personalize this relationship. Still there is, in theory, no restriction on what kind of comment a referee can make on a paper, nor on which of the material an editor receives he chooses to publish and which not. In the end, authors submitting papers to a journal and its readers have not more than a word of good faith of the editors that the material is properly evaluated and selected. While this may generally be the case, and there is always some margin of personal preference (and human error) in the process, it should be unnecessary to point out that the opportunity to abuse such a system is virtually unlimited. As an author, I would hope to, and often do, respect the work of editors (among which I am not) and referees (of which I am one occasionally). Still, all too often I am left wondering which scientific principle can explain what I see.

Ultimately, when mathematical correctness issues got involved, my worries became serious enough to motivate the following report. It centers around a concrete case with a reputed

American research monograph publication M. Before turning to this case, however, I chronologically follow several situations in its background. They link it with the issue of journal publication in several ways and explain my relation to the two authors of the monograph, whom I will call Prof. A and Prof. B below¹.

So far little publicity has been offered in mathematics to this fundamental issue – how to maintain correctness of the literature – and to its relation to journal organization. It is difficult to say to what extent this status quo is owed to lack of occasions where the issues surface, or to the amount of neglect and suppression they experience. There is, however, some evidence that my case is not isolated, while few opportunities are established within mathematical community to debate such problems. This is a reason for me to seek some venue for discussion here.

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2. THE INTERACTION BETWEEN REFEREES AND AUTHORS

Mathematics has a smaller community than the one of most other sciences. This situation can result in exceptional authority granted to a few, as editors have a great deal of discretion and certain individuals are frequently called upon to serve as a referee, perhaps in part because of their high standing in the community, perhaps also in part on their willingness to perform the role.

My experience with Prof. B exemplifies a result of this circumstance. Our initially good relationship deteriorated when he refereed and rejected a submitted paper of mine with the argument that a different proof of my result was written in a (at that time) 7-year old unpublished draft of his. While there might have been other meaningful reasons to argue against publication of my submission, his own failure to write up his results should not be considered as such. I had received a copy of his draft (after submitting my paper) and, without figures, found it unreadable. I then made a fatal mistake of criticizing him directly (thereby revealing to him, of course, that I knew he was the referee). I naively assumed that a senior scientist, whom I also know personally, would be able to react maturely in such a situation. I received no reasonable response from him, but instead another pile of similarly looking comments about my work from various other journals. After this continued for a while, and I saw no other way to defend myself, I put some of these reports on my website, seeking help to make him stop his activity. Of course, I had again to attribute the reports to him, without that an editor would confirm his identity to me – it is a refereeing system that always protects the referee at the cost of the author. (At the time our dispute broke out, B had allegedly declared that he would never again referee my papers.) Although I did (privately) hear some conciliatory voices, my attempts to openly violate anonymity and object to various (also other) referees' actions offended B, and not only him. An editor I pointed to my website reacted as follows.

I am satisfied that the referees consulted were honest and did professional jobs.

If your comments are criticisms of the refereeing process in general, then you should find some other forum in which to debate them.

If they are specific criticisms of [our institution], and you genuinely believe them, then you may do better to find some other journal to submit your articles to. [...]

¹While I keep record of most (written) correspondence from which I quote, I can, for suggestive reasons, not afford to target directly diverse individuals and institutions. I offer, though, to provide this documentation, to serve as a factual basis independent of my interpretations.

I now consider this correspondence terminated.

Is referee anonymity holy enough to teach young and unestablished researchers to fear for their academic survival? B's "fight" has been going on for about 13 years now, and includes a recent case where I solved a problem formulated by himself, and had a coauthor who – like me – was continuously struggling for jobs, but – unlike me – was completely impartial to all this conflict. B's findings that our paper had some weaknesses and the journal was too prestigious might have been agreeable, but there was actually not one single positive word in the report. My coauthor and I received then from another journal a similar report, largely copied word by word from the previous one. I know also of other referees accustomed to such "copy-paste" practices, and I try hard to tolerate them as a "feature" of the reviewing system. Formally, as long as only B's comments on my papers are concerned, this can still go as his opinion and, ultimately, there will be some editor who will not choose him as a referee. Among others, the paper I mentioned he rejected at the time appeared 5 years later, with additions and improvements, in a much better journal. (In comparison, a completed, readable version of his draft was published only recently, almost 20(!) years after its announcement.) In some respects, we may say in this case that the interaction with the peer review system 'helped' me. However, when the career of my collaborators is (also) derailed, and definitely when readers of B's own papers are kept misled (as I will describe in the monograph case in §4), battling me across editorial offices ceases to be a mere private matter.

Prof. A addressed the conflict like this: "He is now your enemy" and "you cannot afford more enemies". First I tried to see this as advice (also from others) about personal relationships. But with A's involvement in the monograph case, I started questioning such a position. Exercised indiscriminately, private hostilities can easily turn into a device for transgressing whatever standards of conduct exist – be it through intimidation, attacking at will, or ignoring scientific disagreement. Who qualifies to be declared an "enemy"? Those whose objections about one's work, or whose own work, one has authority to spread doubt on?

3. QUESTIONS OF EDITORIAL CONDUCT

Questionable practices also occur on the part of the editor. Once, for example, I was sent to review a true (already published) "masterpiece" for the online review database [AMS Math Rev] of the American Mathematical Society (AMS). The paper had something suggested as a "main result", whose proof was 8 lines, with this being the only proof in the paper. The idea was, roughly speaking, to observe that when $a \leq b$, $b \leq a$ and $b = c$, then $a = c$. I did not find anything else really new inside. I perceive such an article as a mockery, not only against myself – I had 9 papers previously rejected by that journal, 8 without seeing any referee report – but also against the efforts of dozens of other authors, who submit much more serious material there, and many of whom are "processed" in a similar way.

In the end, I decided I could not write a review – I saw no way to write anything serious without sounding offensive. Still, I could not refrain from sending an angry letter to the editors, asking how a traditional top-30 journal could accept something like this. (The managing editor was heartfully thanked in the acknowledgment for his help to 'improve' the paper.) For various (personal) reasons, I felt unwilling to raise the issue formally; the editors, however, had little appreciation for that. As I had declared that I might do otherwise in a following case, they referred to these 'threats' as a pretext to collectively refuse to handle my next submission. So while the editors could appear to be comfortable with their doings, it was an outsider who was creating disorder.

Prof. A was following up close my outrage about this paper, and the treatment of mine. From his own experience, he describes a possible editor's position as follows: "If his papers are good, some journal would publish his papers", and "once I decided, it is usually final". So, "[w]hat kind of response do you expect from the editors? Apology?" Still, Prof. A admits that if a decision is made "by a political reason", then "it is hard to prove it". Thus, rather than plausibly addressing editorial difficulties or following scientific guidelines, the opportunity is created to trump up arbitrary ruling. (What proof should expose one? Criminalistic? See also the quote of P. Doty and the end of §8.) However, in response an editor might appeal to a universal (because unobjectable) justification: difference in opinion. Indeed, any perception of the quality or importance of a paper, no matter how many would share it, is subjective – and hence can always be disputed by a particular editor or referee. In the long run, if published material is technically correct, there is no irreversible harm done to mathematics. This is less certain, though, at the presence of errors. Such a situation arose a few years later – and became the occasion for my essay.

4. THE MONOGRAPH CASE

During my research I discovered an error in the monograph of the two authors. It was possible, to large extent, to repair the problem, but it required some explanation. An 8-page note providing the details was prepared and sent to the managing editor (in 6/07) to present the issue to the authors. (I decided not to communicate with the authors directly, after the explained experience, and after having no reply from Prof. A to my previous email regarding an error in another one of his papers.) I hoped for some serious response – that either they could refute the objection, or write an erratum based on the explanation. I received, though, just a very brief comment from Prof. B, sent on behalf of both authors, a few months later. He merely stated that "[w]e never claimed" what I was referring to, and "[i]t seems to us that Stoimenow may not understand details of our paper". It is true that the claim does not appear explicitly, but it enters into the proof of one of the main results. He also sent a 'revision' of this proof, but it had corrected only a few typos. He did not address any of my concerns (which suggested to me that he had not read my note at all, or wished to quickly dismiss it), and I replied listing again one-by-one the questions and objections. To this date I have not witnessed any further mathematical comments from the authors addressing this proof.

First I had submitted, in 8/08, to the same publication M an own long manuscript, in which I included the discussion of the error. Seven months later, in 3/09, a message stated that M could not consider the manuscript, partly due to backlog (of which I had been admittedly advised in advance by the managing editor). Then I submitted my short note as an erratum, assuming that the authors had not written their own. (I was not told otherwise by the managing editor, and it was meanwhile a year and 9 months since my initial indication of the error.) After apparently discussing briefly with the authors, the managing editor, however, declined to handle my correction, too (in particular to have it examined by an independent party). I quote his message:

Corrections are by the same authors. So this must be considered a new article. It certainly is not appropriate for M by length considerations. It certainly seems that you and the authors of the original [monograph] do not agree on many things.

I think that if you want to publish this, you should submit to some independent journal and have it refereed.

So we cannot consider this for M.

He did not explain what issues I disagree on with the authors, and did not ask my position on these issues. I received no response to a further query, asking whether the authors submitted an erratum, and in what form errata to M can be published. Later the same month I met Prof. A at a conference, and he said that Prof. B would contact me regarding the matter. Also, in 2/08, a third person had told me that the authors plan to put a corrected version of the monograph on the arXiv [arXiv, Jackson 2002]. So I waited. After a further ten months, still nothing had happened.

Before I tried submitting the case elsewhere, a senior colleague (whose similar experience I had found only weeks before [Hill 2009, Hill 2010]) advised me to contact the managing editor one more time directly. I repeated my questions to the editor and included the (major, and critical) parts of a draft of the present text that concerned his case. Despite being informed in the draft, among other things, of my conflict with Prof. B, he reaffirmed his determination not to handle (and ask independent opinion on) my correction. He just forwarded to me again the old (and as I explained, completely inadequate to me) response of Prof. B, and declared that "the decision was not to submit an erratum to M".

Only at that point I started sending my correction, *a fortiori*, to other journals. At the time of writing (06/12), after M, at least 8 other journals declined to consider, or rejected my erratum. Only occasionally there are signs of a more careful examination. From the editor of the fourth journal I received the first, so-to-say, authorized word "that there is indeed a mistake in the [A-B] paper". However, according to "a couple of experts", "repairing that mistake is not such a big issue". I do not know whether this, or what else, is a publicly agreeable judgement to close the matter, after all disarray that occurred; for my reasons, see the beginning of next section. The dilemma surfaces more directly in one of the following referee reports:

This paper describes an error in a [monograph] that appeared in [year]. Given that the [monograph] has been cited [X] times (according to the MathSciNet count) it is not a major publication. Nonetheless, it is of interest to the [specialized] community and has generated further research. Thus a paper correcting an error in it should be published. But does [your journal] publish papers of that sort, or should there be an audience outside of [this particular] community?

In summary, 5 years after I originally raised my objections to M, I still have seen no effort of others to address (or to allow me to address) the issue officially.

5. MATHEMATICAL LIT(T)ERATURE?

Flaws have been found also in diverse other papers, and that some authors and editors are unenthusiastic in considering these is hardly novel, also to myself. The reasons I attached some significance to the problem with the monograph are that

- * this is one of the main results of the monograph,
- * there is no (to-be-corrected) reprinting of research monographs scheduled (unlike e.g. undergraduate textbooks, to which one may tend to compare M rather than to research journals),
- * the error is subtle (it had not been noticed for about 15 years since the manuscript was written),
- * this is the only proof available,

- * other work (published also in other papers) depends essentially on this result, and
- * I know at least one other (published) paper which uses the argument and may (without that I went into details there) encounter the same problem.

Of course, I can unlikely win a dispute about subjective views on importance of corrections. But while it is suggestive that one shouldn't necessarily write an erratum for every typo, it is hardly convincing when I am left to wonder whether my explanation of the error was carefully read and understood. There is some consensus in the scholarly community that it is customary the original authors to write an erratum, but in part we may also consider this to be their (and not my) duty.

What justifies such authors' conduct? Unlike possibly in tabloid journalism, an argument in science that something (here, a correction) is redundant to publish because presumably not of great interest makes little sense: should we write no proofs because the fewest read them? Do the authors reckon that I better not "[dis]agree on many things" with them, when I "cannot afford more enemies"? Or do they find in their great expertise confidence not to take seriously my concern? Lang writes [Lang 1993, end of §V.4]: "I do not recognize being an 'exceptional scientist' as a license to throw one's weight around to avoid answering scientific criticisms." I see his reasons. In the end, if littering around in one's publications is a negotiable freedom, who will regulate what and how to be traded for it, and who will organize the cleanup? Many researchers know that finding a correct proof of a non-trivial statement is often painstaking. How will one be motivated in going through this when seeing others well received with such working attitude?

Another point to keep in mind is that, even if a journal does not remunerate its contributors, publications are not a charity enterprise. There is thus little reason in expecting their correctness to be one, instead of holding authors personally accountable for what they seek employment, tenure or grants upon. However, as noted in [Lang 1993, V.4(b)], in reality notorious cases throughout science (see, e.g., [Buzzelli 1993, Odling-Smee et al. 2007]) arise with established figures, to whom career and funding pressures are, at least, not existential. Such individuals know well that few of the affected can afford an open conflict and, if a conflict indeed arises, can take advantage in it. Most people naturally stay away from disputes, rather than investigating what whose merits or stakes. Others go along with a scientist's prominence, or at least are reluctant to openly and actively take position against him (and whatever negative publicity is far more critical to the less established ones). Someone wrote to me regarding my case: "I just want to be friend with all involved parties." This is just, unfortunately, not what readers seek in one's papers.

One other defense of one's conduct is to refer to the existence of courts of law. Lang writes in [Lang 1993, §V.3] that "[s]uch a point of view undermines the exercise of scientific responsibilities, as distinguished from legal responsibilities." This way practices like sloppy writing, refusal to correct errors, or lousing around on junior colleagues, become acceptable, because they are *not illegal*. Similar is an argument of the sort 'this is how it goes'. Accordingly, corruption in mathematics is legitimized when a majority of mathematicians becomes (or because it already is?) corrupt. (See [Lang 1998, p.447-451] for a high-profile instance of such reasoning.) In many conversations Prof. A (and not only he) has discouraged me from (and criticized others for) settling scientific disputes legally. I find, though, increasingly less reason to share his optimism about academic authorities' self-policing. In result, I feel continuously pushed toward a structure of behavior, where making a 'career' means to let go whatever

higher ups please, in order to keep a chance of being allowed one day to do similar with others.

One might try to imagine objective reasons for such an indefinite delay, in that an author, as a human being, is subject to change of duties in life. Much less so, however, is a journal (or monograph series). This is why it is a plausible thought that it should be ultimately the journal that has to properly address issues of the correctness of the mathematics that has appeared in it, and to provide ways for corrections to be published.

6. THE ROLE OF JOURNALS

There seems no definite codex of editorial conduct, and so editors have some freedom to set their own policies. But whatever these may be, they should not oppose basic scientific principles.

Is it reasonable that potential rules regarding such matters as authorship of errata, or adopting the authors' position in a dispute with readers, are to take precedence over issues of correctness of mathematics? If the journal cannot guarantee the author's responsible involvement, it should grant the right to others to publish corrections, and certainly have them carefully – and independently – considered. For example, in the Hales-Hsiang case [Hales 2006, Szpiro 2003], the *International Journal* did publish Bezdek's counterexample [Bezdek 1997] to one of Hsiang's main claims.

Such a practice follows a basic tenet of science about the dissemination of criticism. This is seen also in the principle that an author should not referee his own paper: he can never be deemed objective about his own work. Then similarly he can hardly always be objective about its correctness. If solely an author should write, or judge over errata to his paper, is such a rule there to manifest truth, or authority? In fact, Prof. A himself advised me against complacency as an author, writing in connection to my experience in §3: "You should be modest. You believe your paper is excellent, [...] but others may not think so." This is certainly true, but it makes his attitude towards his own case appear that much more strange.

There is, however, indeed a difference between critique of published and unpublished work, which becomes obvious from the journal's point of view. No journal is responsible for an unpublished manuscript, and in fact journals seek ways to dispose of submissions. Whether unable or unwilling, editors are seldom concerned about legitimacy of critiques toward authors, and generally protect their referees (as seen in the editorial quote of §2). This has led certain referees to the conviction that peer review is a good arena for hitting on "enemies" as one pleases (instead of, but doing as if discussing mathematical flaws). The situation changes, though, if a paper is published. On the one hand, some authors see in a publication by a reputed journal, however achieved, a certification of supremacy. On the other hand, problems arising with its publications directly affect the journal. Thus a new culprit must be sought, who is easily found in the audience. It is not surprising, therefore, that when authors rebuff an objecting reader, some editors happily join the chorus. The result is to enforce deciding how "excellent" one's paper is by whether and where it is published, while discouraging and obstructing its actual study.

Another issue raised in the M editor's letter to me was length. I find it a rather questionable argument that form should have priority over content, and that for length reasons M cannot publish a single page of corrections, when it publishes hundreds of pages of research per year. Who is served by preferring long papers to correct ones? Or are all M's papers ascertainably error-free?

A journal declaring a published paper to have undergone some refereeing is very far from a guaranty. Even with the purest (and only very occasionally present) intention to examine a paper mathematically – and not politically – a referee is not infallible. Often he sees well his shortcomings and recommends publication relying on authors and journal to take ultimate responsibility. This makes sense, since neither can he take such responsibility, when his work is not publicly disclosed, nor should he, when he acts voluntarily and is not supposed to receive any credit for a publication. Thus pronouncing refereeing a stamp of correctness for everybody to believe in is not more than some editors' absolutory tale.

7. COMMON EDITORIAL PRACTICES

Of course I have to respect editors of a mathematical class much higher than my own. Also, we should be inclined to believe that many of them do their best to make a journal for their readers as good as they can. It is plausible that many submissions, diverse referee opinions, a lack of definite evaluation criteria, etc., pose difficulties to the publishing selection process of a journal. On the other hand, an author's life is hardly made easier. A journal demands exclusive right to handle a manuscript, which is not to be submitted simultaneously anywhere else, but it guarantees no time frame for this. Delays to consider a submission (or even not to consider it, as in my example) are not uncommon. Neither are comments of a referee written without any degree of politeness. Even with a positive referee report, or after the author is requested to submit a revision, the final decision can well be negative, and a debate over it is deemed useless. At the same time, the author may need to seek employment, or to compete with several other researchers working on the same subject.

However allegedly essential, freedom and severity of editorial action appear to me particularly objectionable in the case of corrections. One common practice is not to give any serious mathematical explanation when declining publication. Two types of argument are often, implicitly or explicitly, appealed to in such a situation: the limits of printing space, and the journal's control over quality. However, over corrections such phrases not only, as discussed in [Lang 1993, §II], prevent meaningful scientific debate, but they appear questionable even from the point of view of mere common sense.

If being merciless over submissions is meant to achieve quality, it is not clear what notion of quality is attained by leaving errors unfixed (and improperly examined). I hereby clearly separate quality from bibliometrical rankings or the like [Bornmann-Daniel 2008, IMU 2008, Ewing 2006], which seem increasingly perceived as a measurement for it – and which have already documentably led to worrisome editorial conduct [Mushtaq 2007, Arnold 2009]. Neither is it convincing that errata, which usually occupy a minor part of the printing space, are mainly responsible for the pressure of publishing backlog. When journals (agreeably) declare upon rejecting papers that they cannot publish everything worthwhile, why do they still prioritize new material to corrections? Are impact factors at the basis of gradually turning the reliability of the literature into a liability?

When any scientific reasoning fails, the ultimate argument to silence all dispute is, as explained, that the editor's (or referee's) *opinion* decides what material is suitable for the journal. Conveniently, one is then commonly pointed to other journals as a possibly more appropriate place for one's work (see for this below). However agreeable or not, such rhetoric might make sense as far as it concerns new research. But when a journal's opinion is valued enough to justify it turning away and requesting its own errors to be addressed somewhere else, I see nothing at all that could not be justified. What will it start looking like when, according to

its opinion, every journal can feel free to do and let publication of whatever it pleases? Is the order to the occurring chaos to be sought throughout the publishing landscape, the internet, or in someone's private conversations?

The difficulties with publishing in "some independent journal" are evident from the reactions in §4. One other journal quoted a referee calling my erratum directly "a strange paper". Its purpose was indicated, and its story had been explained to the editors at submission. Whatever their form or importance, I do not see what should be so "strange" about corrective efforts. If a 'normal' way to judge scientific material is that the statement matters, not the proof, is this how one is concerned about what appeals to the journal's readership?

I admit that there are recommended, and possibly more appealing (but far lower than M in political stature) places where I have personal reasons not to submit my correction. This is not unusual. The publishing process has now taken essentially every right from an author, except one: his choice of journal. Even this sole freedom is constantly targeted by attempts to manipulate authors where to submit. I say 'manipulate', because such efforts are by no means always in the author's interest. Many of these recommendations may not be intendedly adversary, but neither are they compellingly serious and sincere. My experience has shown it *vital* for my publishing activity that I almost *continuously disregarded* such advice.

Of course, this may result in an author's failure to have his work properly published. In such a case, an attitude taken in the publishing system, as seen from A's quote in §3, is that an author is responsible for securing attention to whatever he does. Even for a correction, the reply from M directs the question toward whether "[I] *want* to publish this" [emphasis added], and away from whether or what *they should* do. Had A, B or M taken proper action, or would the diverse people aware of the case still convince them to do so, then submitting my note, and the whole discussion about who and where to read it, will be unnecessary. However, while these same editors and referees bounce around a correction like much other material, few of them seriously entertain "the fundamental problem of scientists not answering scientific criticisms of their work, not allowing publication of criticisms, or requiring other scientists to submit to various authorities" [Lang 1993, §3]². No matter how plausible their attitude, the result is to make journals virtually "clogged" as "ordinary scientific channels [...] for the presentation of scientific challenges". (*ibid.*)

As indicated, many problems arise also when treating the internet at large as such a venue. Lacking an appropriate level of reliability, and deployed as a retreat from publishing responsibility, it will only deteriorate standards of communicating research. When a correction is available *somewhere* on the internet, can it be readily located? Will it be permanently maintained? Who stands for its accuracy? And most importantly, again: will those who published the mistake actively engage in settling these issues, or will they believe it – and leave it – to be someone else's job? Or when "a couple of experts" are aware of every problem, will they make effort to honestly and consistently spread the information, and will they reach anyone who potentially needs it? Not only my case has shown many "experts" concerned about other things than mathematics.

8. CONCLUSION: PRIVATIZING MATHEMATICAL CORRECTNESS

Over a long period Prof. A has repeatedly suggested to me that problems are better quietly entrusted to those in charge, and that challenges disturb the common atmosphere. There

²A colleague quoted an editor writing to him: "Your paper is unsuitable for publication because it corrects another paper."

might have been the needed sense of responsibility around in scientific community in the climate he grew up 40 years ago to make this a workable attitude. But I see such responsibility neglected now. Applying Prof. A's principle to his own case, I waited almost 3 years before objecting publicly. What I have seen during that time is inaction, scientific misrepresentation, and arbitrary power.

I do not agree that authors who deem me an "enemy" or that I "cannot afford more enemies" should not seriously discuss their flawed mathematics. I clarify again that I cannot judge the importance of my correction, and that it would have been unnecessary, had mathematical arguments shown otherwise, or had the authors themselves handled the matter properly. But I do maintain that correctness issues need open (and not only the authors') evaluation and jurisdiction. And I do maintain that the proper way to answer criticism (mathematical and ethical) is not that, "like the video games[,] one can't shoot fast enough" [Lang 1998, p. 797] at (job-seeking) "enemies". I feel not satisfied with scientific achievement, or the internet, as an exemption from publishing responsibility. And I do not see covering up flaws in papers as a good way of polishing journals or research resumes.

Without a job for several years, I have repeatedly found people with decades of professional experience, including such who advised me regarding my juvenile behavior, showing looser manners than my own. There is reason to condemn my indignant reactions in various situations, as long as some minimum on academic values is respected. But when I fail to see anything about mathematics to stay out of all the political intrigue-making, those responsible for maintaining standards have turned it into an uphill battle. And this problem neither occurs only due to, nor does it affect only myself. G. Perelman, despite being widely respected for his work, retired from mathematical research, and was quoted regarding his decision [Nasar-Gruber 2006]: "It is not people who break ethical standards who are regarded as aliens. It is people like me who are isolated. [...] Of course, there are many mathematicians who are more or less honest. But almost all of them are conformists. They are more or less honest, but they tolerate those who are not honest." I consider this an excellent summary of the state which policies of indulgence, pleasing and fighting each other in mathematics have led to.

Thus I wrote my account down, in order to lend importance at least to this question: *Should one tolerate that authors and journals can evade involvement in correctness issues of their own publications?* I fear that if this becomes widely adopted, journals will be there for propaganda, while the introspection of mathematics will be relegated to the gossip. And mathematicians will be tempted to politically smuggle papers past scientific control. In relation to the Baltimore case, P. Doty [Doty 1991] wrote that such "attitude towards the responsibility of authors [...] is a critical departure from common standards. . . [T]o leave to others the responsibility of establishing the validity of what you have published is not only a fundamental retreat from responsibility but, if it became accepted practice, would erode the way science works. For [...] science moves forward by building rapidly on what is published on the tentative assumption that it is correct, not by waiting for others to test each paper's validity."

Indeed, while I cannot seek public attention to every case I consider annoying, I do not see much decency coming up ahead with (what is supposed to be called) a "publishing system" looking like this. Mathematics can only be done by human beings, and may carry their imperfection. But is it still felt as a common duty, or is it now a private property? "In this way we risk sliding down toward the standards of some other professions where **the validity of action is decided by whether one can get away with it** [boldface added]. For science to drift

toward such a course would be fatal – not only to itself and the inspiration which carries it forward, but to the public trust which is its provider.” (Doty *ibid.*³)

9. POSTSCRIPTUM: WHAT TO DO?

As a response to my letter to the AMS [Stoimenow 2010], I had some discussion with Michael Fried, who pointed me to his article [Fried 2007]. In order to improve refereeing standards, Fried proposes a pool of special referees for high-quality journals. He believes that “behind considerable corruption is a community neglect of developing significant mathematical skills”, which should be promoted (or preserved) by educating such reviewers. These referees should be public and receive a small payment. I insist that “public” should mean disclosing the referee’s identity for a particular paper, and in case the paper is accepted, the referee can share a minor part of credit and responsibility for publication. I am strongly convinced that, as long as referees are kept anonymous, a considerable improvement in their ethics is not in sight. Thus paying anonymous referees will only aggravate the problem. I have also some reservations toward a blind-refereeing approach (where the author is unknown to the referee). This will pressure authors to write papers with an aim to conceal their own identity. Such attempts can still often be easily uncovered (in particular, through the internet), and will only go at the cost of presentation.

Another point is the need to establish a general administrative process to deal with issues of correctness (both ethical and scientific) in mathematics. According to a familiar source, the AMS has no such channel, and I know of nothing similar at other mathematical institutions. But my case is not isolated (see, e.g., the mentioned articles by T. Hill), and there exist structures in other sciences to address such problems, which suggests that these may deserve more serious attention in mathematics, too. I point out again, following [Lang 1993, §V.3] (as quoted in §5), that civil and academic responsibilities are two different things, which is why challenging scientific conduct by legal procedures not only bears enormous risks, but also questionable prospects. The lack of scientific procedures has created a “legalistic morass” (Lang *ibid.*), where the idea blossoms that as long as “it is hard to prove it” for the “enemies” what one does, all should be considered legitimate.

Ultimately, whatever system is devised, I see the central point to be, as S. Lang writes in [Lang 1993, Conclusion], that scientists “uphold the traditional standards of science.” And in doing so, “[t]hey must rely on individual responsibility, and they must create an atmosphere and conditions under which scientists, both young and established, can exercise this responsibility without fear – fear of retaliation, fear for their careers, fear for their funding, fear for their publications, fear of the tensions which come from a challenge, fear of being uncollegial, whatever. Will they?” (*ibid.*)

REFERENCES

- [AMS MathRev] *American Mathematical Society Mathematical Reviews (MathSciNet)*, <http://www.ams.org/publications/math-reviews/math-reviews>
- [Arnold 2009] Arnold, Douglas N. (2009), *Integrity Under Attack: The State of Scholarly Publishing*, SIAM News, December 4, 2009, <http://www.siam.org/news/news.php?id=1663>
- [arXiv] *arXiv.org e-Print archive*, <http://arxiv.org/archive/math>
- [Bernstein] Bernstein, Dennis S., *A Student's Guide to Peer Review*, http://www.et.byu.edu/~beard/Helps_for_students/peer_review.pdf

³Lang, who has extensively quoted Doty’s piece, comments on it on [Lang 1998, p.339] that “we do not ‘risk’ sliding down toward such standards; we have reached them.”

- [Bernstein2] ———, *On Review Practice*, <http://aerospace.engin.umich.edu/people/faculty/bernstein/guide/ReviewPractice.pdf>
- [Bezdek 1997] Bezdek, K. (1997), *Isoperimetric inequalities and the dodecahedral conjecture*, *Internat. J. Math.*, Volume 8, Number 6, 759-780.
- [Bornmann 2011] Bornmann, L. (2011), *Scientific peer review*, *Annual Review of Information Science and Technology*, Volume 45, Chapter 5, <http://www.lutz-bornmann.de/>
- [Bornmann-Daniel 2008] Bornmann, L. and Daniel, H.-D. (2008), *What do citation counts measure? A review of studies on citing behavior*, *Journal of Documentation*, Volume 64, Number 1, 45-80.
- [Buzzelli 1993] Buzzelli, Donald E. (1993), *The Definition of Misconduct in Science: A View from NSF*, *Science*, Volume 259, Issue 5095, 584-585, 647-648.
- [IMU 2008] *Citation Statistics: An IMU Report* (2008), *Notices of the American Mathematical Society* Volume 55, Number 8, 968-969.
- [Doty 1991] Doty, P. (1991), *Responsibility and Weaver et al.*, *Nature*, Volume 352, 18 July 1991, 183-184.
- [Ewing 2006] Ewing, John (2006), *Measuring Journals*, *Notices of the American Mathematical Society*, Volume 53, Number 9, 1049-1053.
- [Fried 2007] Fried, Michael D. (2007), *Should Journals compensate Referees?*, *Notices of the AMS*, Volume 54, Number 6, 585, <http://www.math.uci.edu/~mfried/proplist-ams.html>.
- [Hales 2006] Hales, Thomas C. (2006), *Historical Overview of the Kepler Conjecture*, *Discrete Comput. Geom.*, Volume 36, 5-20, DOI: 10.1007/s00454-005-1210-2
- [Hill 2009] Hill, Theodore P. (2009), *How to Publish Counterexamples in 1 2 3 Easy Steps*, <http://www.scribd.com/doc/19819297/How-to-Publish-Counterexamples-in-1-2-3-Easy-Steps>
- [Hill 2010] ——— (2010), *Hoisting the Black Flag*, *Letters to the Editor*, *Notices of the American Mathematical Society*, Volume 57, Number 1, 7, <http://www.ams.org/notices/201001/index.html>
- [Jackson 2002] Jackson, Allyn (2002), *From Preprints to E-prints: The Rise of Electronic Preprint Servers in Mathematics*, *Notices of the American Mathematical Society*, Volume 49, Number 1, 23-32.
- [Lang 1993] Lang, Serge (1993), *Questions of Scientific Responsibility: the Baltimore case*, *Ethics and Behavior*, Volume 3, Number 1, 3-72, http://www.gatewaycoalition.org/files/Gateway_Project_Moshe_Kam/Resource/DBC.html
- [Lang 1998] ——— (1998), *Challenges*, Springer Verlag, 816 pages.
- [Mushtaq 2007] Mushtaq, Qaiser (2007), *The Misuse of the Impact Factor*, *Opinion*, *Notices of the American Mathematical Society*, Volume 54, Number 7, 821.
- [Nasar-Gruber 2006] Sylvia Nasar and David Gruber (2006), *Manifold Destiny. A legendary problem and the battle over who solved it*, *The New Yorker*, August 28, 2006, http://www.newyorker.com/archive/2006/08/28/060828fa_fact2
- [Odling-Smee et al. 2007] Odling-Smee, Lucy; Giles, Jim; Fuyuno, Ichiko; Cyranoski, David and Marris, Emma (2007), *Where are they now?*, *Nature*, Volume 445, 244-245. DOI: 10.1038/445244a
- [Stoimenov 2010] Stoimenov, Alexander (2010), *Honesty in Mathematical Writing*, *Letters to the Editor*, *Notices of the AMS*, Volume 57, Number 6 (June/July 2010), 703.
- [Szpiro 2003] Szpiro, George G. (2003), *Kepler's Conjecture: How Some of the Greatest Minds in History Helped Solve One of the Oldest Math Problems in the World*, Hoboken, NJ, John Wiley & Sons.

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